

Jeffery, Geoffrey 2005

Dr. Geoffrey Jeffery Oral History 2005

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Geoffrey Jeffery Interview

Office of NIH History Oral History Program

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Leo Slater: This is Leo Slater. Today is January 12th, 2005. I'm here interviewing Dr. Geoffrey Jeffery at his home in Decatur, Georgia. This is part of my Stetten Fellowship on the history of malaria research at NIAID and I just want to confirm to you Dr. Jeffery that we will be taping our conversation today.

Geoffrey Jeffery: I understand. That's fine.

LS: Thank you. I thought we'd start in your early years with your family background; you grew up in – partly in New York, partly in Connecticut. Where were your family from?

GJ: That's right. Well I was born in a little town up in western New York State, Dundee, New York. My father was raised out in Iowa. His father was a Methodist minister, a circuit rider initially in the late 1800s in north, northwestern Iowa. He did circuit riding up in the Dakota territories before they became a state and he had immigrated from England in about 1875 or '80, somewhere around there, went back and got a wife and took her out to Iowa and raised a pretty good sized family and my father was raised there as a minister's son and went on to get a Master's Degree at Washington University in Saint Louis and became a teacher and then finally a superintendent of schools in Bridgeport, Connecticut. And Connecticut was where I was raised mostly. I was born in upper New York State, but when we lived in Milford, Connecticut, which is between New Haven and Bridgeport for many, many years and stayed there until I left for graduate school.

LS: Were you interested in science as a kid?

GJ: Yes I was, more or less. I think my mother was pretty much interested in having me become a scientist. Maybe I showed some bent for it, and in high school I was fortunate enough to become associated with a biology teacher who had a, I guess you'd call it, a select group of young men that he brought in every afternoon after school and they went out on field trips and I was able to join that and got very much interested in it. And when I went off to college I sort of intended to go on for medical -- to the medical profession. It didn't work out immediately, but --

LS: What kind of field trips? Like collecting and...?

GJ: Collecting, yeah, I always remember one of the evening papers in Bridgeport, the school was in Bridgeport, it was a picture appeared -- that showing three boys and the instructor out holding up copperheads. We used to go hunting copperheads up in the hills back in Connecticut. She had no idea I was doing that sort of thing.

LS: People probably don't do that much anymore. So then you went to Hobart?

GJ: Hobart College, yeah.

LS: In Geneva?

GJ: Geneva, New York. I had a brother and sister and these were Depression days; school teachers didn't make much money to start with and then they cut their pay pretty well during the Depression so we got a deal from Hobart College for all three of us to go at once. My sister had started off at Duke University and went two years there and unfortunately for her, I think, she had to move to William Smith College which is the coordinate women's college with Hobart. And my brother had been to junior college for one year and then the three of us went on to Hobart together and I was there for four years and finished in 1940.

LS: What did you major in?

GJ: Biology and chemistry.

LS: Anybody stand out the way your high school science teacher stood out there as far as...?

GJ: Not particularly. My biology teacher – well there was some who were fairly stimulating, but most of them were not what I'd call stimulus in the [unintelligible], but I can't blame my college record on my teachers.

LS: And then you went from there –

GJ: To Syracuse University, yeah. I had planned to apply for medical school after college and went looking back at my record. My first two years of college were sort of a miserable academic mistake. I got through, C's and that sort of thing, and that wasn't going to get me into medical school. My second two years were much better, in fact I got mostly all A's in my final year. That got me into Syracuse graduate school in zoology and so I went there for the year 1940 / 1941 to get a Master's Degree thinking afterwards that I might then apply for medical school. And my career at Syracuse was academically very good and personally it was very good. I met my wife there and I became associated with Dr. Reginald Manwell [spelled phonetically].

LS: So he was already there when you arrived?

GJ: Yeah he was there. He was a graduate of Johns Hopkins and had gone to Syracuse and was teaching zoology and he -- I started out my graduate – am I going into too much detail? Don't let me wander too much.

LS: No you're doing a good job. I'll keep you in line don't worry.

GJ: I started out – I had to pick – I wanted to get through the graduate school in one year, get a Master's and get out and go on with my life after one year. Usually it took about two to get a Master's, but they allowed me to at least aim for one. I got very good grades in my coursework and I had selected originally a project, a research project, in comparative anatomy. I had a very good professor there, Dr. Struthers [spelled phonetically]. He had a very nice looking daughter. I thought that was a good place to be, and he talked to me and got me started on a project and he presented me with a preserved shark and he said, "Now you will follow this cranial nerve all the way from up here down to where it ends and that will be your project. During the year you will dissect that thing out and catalogue the whole thing." Well I think that I looked at that thing for about half a day and I said, "Well I'm not going to spend a year on this damn preserved fish." I found out that Dr. Manwell needed somebody to help him take care of his canaries. He was working in bird malaria a lot, and I went and talked to him and he said yeah he could take me on as a student. He had a project sort of worked out that I could work on and get through within a year if I would be willing to take care of his canaries on the weekends, feed the ducks and that sort of thing, he would take me on. And so I went with Dr. Manwell, which probably was the key decision in my professional career. He –

LS: Did he have a lot of canaries and ducks?

GJ: Oh, he had a room about this size, maybe two-thirds this size with cages all of canaries. He may have had as many as 50 to 100 canaries at a time, and he didn't really work with ducks until I got there because I started – my work was going to be with plasmodium *lophurae*, which was carried – it was transmitted in ducks. I think it was originally a parasite of the pheasant from South East Asia, but it went in ducks very well and it was a very good parasite because it produced high infections and these invariable infections, and the projects that I got into was starting right from scratch was trying to preserve malaria parasites by freezing, viable parasites, by freezing and that carbon dioxide which wasn't – solid carbon dioxide temperatures - 78. I had to build myself a cabinet to hold them in, got an old coca-cola cabinet and then re-insulated with it with stuff going around, and then started my dry ice experience and basically that was successful and I was able to freeze the parasites. It had been done before, Dr. Lowell Cogalshaw [spelled phonetically] down at Rockefeller Foundation in New York had done some of this work and I went down to visit him, got to see the Rockefeller Foundation. That looked like a good future, too.

LS: Do you remember anything about Cogalshaw?

GJ: Yeah, quite a lot. He was rather tall, very friendly congenial sort, and to spend the time talking to a green student who just came to get information, he was very cordial and very nice. And then I met him again over the years briefly. I never had anything really – I was not associated with him or the work that he did, but knew what it was he was one of the people that recognizes me as one of the leaders in the field.

LS: Did you or Professor Manwell research mosquitoes?

GJ: I didn't work with mosquitoes there. He did a little with that work and I learned a bit about how to breed the – how to raise *Aedes aegypti*, and Culicidae [spelled phonetically]. We didn't work much with anopheles because the infectors of bird virus were the culicine mosquitoes mostly. He – well anyhow I worked with him for – I got my degree, finished my final oral I guess it was in August and in the meantime he had suggested to me that I might want to go on for a doctorate somewhere, which I was open to, and I began to think that was the better course than medicine for me because I was very much interested in this type of research and he had connections to both Johns Hopkins and the University of Chicago. Chicago was where Tolliver [spelled phonetically] and Huff and a number of people were who all had been in school with Manwell, and of course back at Hopkins Robert Hegner [spelled phonetically] was still professor of protozoa zoology there and he had connections there and he – I applied to both of them and he sent letters of recommendation. I was accepted to both of them and since I was an East Coaster I decided to go to Hopkins which was a good decision.

LS: I think this is the paper –

GJ: Yeah. That's my first publication.

LS: Your thesis with Manwell and –

GJ: That's right, uh huh. That's the first one.

LS: Interesting. I mean this sort of thing is important because otherwise how can you share research materials, how can you preserve strains to compare them and –

GJ: Well later in my career, much later in my career, I went on with the human malaria parasites and we published that a number of times and the preservation of the human parasites.

LS: At Hopkins you mentioned Robert Hagner. You started work with him before he passed away in '42.

GJ: Yes, I did. I went there in September of 1941 to work under Dr. Hagner in his department, and of course I hadn't selected any particular project. The first year mostly was course work and mostly I was having to pay for it myself because I didn't have an assistantship at that time, and Dr. Hagner, when I got there, it was obvious that he was fairly ill. He did still do his lectures and attend his laboratories and he was just still as sharp and just as funny. I mean he had a great sense of humor and very tragically I think he died in February or March, I think, of '41 or '42.

LS: Yeah, that sounds right.

GJ: And so I was not quite as unfortunate as one of my colleagues there who – he had worked – he started out at the school working for Dr. Raymond Pearl and he died. He transferred over to Dr. Hagner's department and he died. This was Myron Simpson [spelled phonetically] he was a nice fellow. He was adaptable. He finally ended up working with Dr. Cork [spelled phonetically] finished up his degree.

LS: That's not a good graduate school experience.

GJ: No it's not but people do die.

LS: No, no I –

GJ: I started out at Hopkins just as a student, took a lot of courses. Dr. Hagner was a very good mentor, even the short time I was there. They admitted me with certain provisions that I had to take histology, embryology, pathology and a number of things because I didn't apparently have enough of it in my background to suit them. Dr. Hagner went before the faculty committee and says, "You're not telling me what my students are going to have to take. I'll tell them what they have to take," and they agreed so I got relieved from all those things, which might have been good, might have been bad, but I did take the pathology, which he thought anybody should have. But he was a very nice person, very nice kindly person, and his wife was very nice to the – I was married in August and my wife and I went to Baltimore to start and it was a good experience. We were very poor. We didn't have any money, but my wife worked across the street at the hospital, in the history room I guess they called it, and she was paid the magnificent sum of \$50 a month, but she got her lunches which she said she couldn't eat.

LS: So she was in the Welch Library?

GJ: No she was in the – well actually in the administrative office of the hospital, but they called it the history room. They stored the histories, the old histories and that sort of thing, and worked in the emergency room on occasion just to have a job that's all. It was a nice experience for her, too.

LS: So after Hagner passed you worked with Lloyd Rosebloom [spelled phonetically]?

GJ: Yup.

LS: And did that change what you were going to do at all?

GJ: Not necessarily. I think it might have. Dr. Hagner was more interested in the malaria parasite, per se, and the biology of the malaria parasite and the relapse patterns and everything that has to do with its life history, and Rosebloom was, of course, more interested in the transmission of the parasites by mosquito. And so I got into – I guess I got to be a medical entomologist in some way because he was a medical entomologist, but I always considered myself a parasitologist.

LS: So what kind – you worked again with *lophurae*?

GJ: Yes and – yeah mostly with *lophurae* and the project with the mosquito transmission with *lophurae* which had not been explored very fully at that time. And so we – that was my major effort -- still have my thesis in there, dissertation we called it then.

LS: Yes. You mentioned Myron Simpson and I just wanted to throw out a few other names of grad students and if you have some recollections of them. Reginald Hewitt [spelled phonetically].

GJ: Hewitt he had left by the time I got there and was working for American Zion [?] – or one of the drug companies up in Connecticut. Reginald Hewitt came down – I met him a number of times. Oh, I wasn't very close to him, but we talked on a lot of occasions and he was of course very much interested in bird malaria, because he wrote the book.

LS: Literally wrote the book.

GJ: And – but he had left by the time I had got there but returned every once in a while. I got to talk to him and see him.

LS: Marion M. Brook [spelled phonetically]

GJ: Marion Brook, yes he had – I think he finished his degree, I think, the first year or so that I was there and stayed on as an instructor and was there the whole time I was, and I've known Marion for many, many years and followed his career and mine sort of merged at least once. He went up to Memphis, Tennessee and was in the department of medicine out there and they had a fairly good parasitology group out there. And then, when CDC was formed, they were putting together a laboratory facility and they recruited Marion Brook to come and head that laboratory facility out at CDC, the brand new CDC. Well actually it was still MCWA when he came, and on his way back from Memphis we were out – at that time I was stationed in Sheffield, Alabama. He stopped and spent a night with us there and asked me if would like to join him in Atlanta in this new thing. I said, "Yes, I would." I was kind of tired of wading around the swamps and counting particles on glass slides. We were doing a lot of insecticide, aerial insecticide work and that sort of thing. So that's how I eventually got back into the laboratory part of the profession.

LS: And Robert Wrendorf [spelled phonetically] is somebody else you –

GJ: Bob Wrendorf, I knew him very well and here again his career and mine have merged and followed each other. He was – I always remember my wife saying that she thought I was tall and the first day we went back, or we went to Hopkins, and we met Bob Wrendorf; he's probably about six inches, a lot taller than I am, at least. A tall guy, a very nice fellow, very good – but he graduated from Hopkins – got his SCD under – I guess also under Rosebloom and then went right into medical school at Hopkins and finished his medical degree there. A lot of them did that. In fact, if I hadn't gotten drafted I would have too. He was a very good person. I worked with him and he and I published together, oh, in the late '50s I guess, additional work on the freezing of [unintelligible] parasites.

LS: Did you know a tech named Evelyn West?

GJ: Evelyn West, yes.

LS: She –

GJ: She finally married one of the students there Bob – he went out to Chicago and worked with – and the group out there. Bob somebody or the other.

LS: Well I can probably figure it out.

GJ: Evelyn West was – she was Dr. Kegner's [spelled phonetically] technician when he died. One of his technicians, I guess maybe she was the main one, and she stayed on and worked for, I guess, for [unintelligible] Wilson and for others that were doing the same things.

LS: Dr. Wrendorf, I show him working at Seagoville in Texas.

GJ: Yes, yes.

LS: Did you have any contact with the Seagoville –

GJ: Oh yeah, yeah.

LS: -- people?

GJ: Yeah that was –

LS: -- sort of out of order, but –

GJ: -- quite a lot later when the NIH group had the programs of inoculating malaria patients in penitentiaries and prisons and doing drug work on the prisoners. One of the places they used was Seagoville. It was a federal correctional institute down in Seagoville, Texas somewhere between Dallas and Fort Worth, and I went out there several times carrying mosquitoes for inoculation. But about the time that the malaria work was winding down they thought that this would be a good place to look at the possibility of other parasitologic subjects and they looked at the inoculation of patients with known quantities of intestinal parasites, giardia, I don't know if [unintelligible] was included in that or not, but I can't remember which ones they were but they did quite a long study on that and I think it was fairly successful. It didn't last very long, and then Wrendorf was transferred back to Columbia, South Carolina to work under Dr. Martin Young there and about the same time that I was transferred from Milledgeville to Columbia, South Carolina so we worked together for several years in Columbia.

LS: Just a few of the professors at Hopkins. You mentioned Cork [spelled phonetically].

GJ: Yes.

LS: Did you work – did you take classes –

GJ: Oh yeah Dr. Cork was head of the department. They merged the helmathology [spelled phonetically] with protozoan zoology and medical entomology fell into it. He was head of the whole, I guess, department of parasitology after Dr. Hagner's passing or sometimes after. I don't know how soon it took place, and he was a very fine person and a very capable teacher and administrator. I didn't do any research projects under him, but I took a lot of courses from him. He taught most of the advanced courses in parasitic diseases, exotic diseases and this sort of thing. He was more interested in the digenetic trematodes than I was. I couldn't get a lot of enthusiasm about digenetic trematodes.

But I do remember very well during my course of being at Hopkins the war had started. I had somehow developed two children, which will happen I guess, and things were really tough. I was working two jobs outside of school and being an assistant at school and my wife went to talk to Dr. Cork and told him that I was going to – I thought I might have to quit and go get a job somewhere and he said – he finally – he said he'd help me do anything I needed to. He said he didn't want to advise me to leave. He said a thing that always stuck with me. He said one thing that he wanted to make very clear that if you leave now with the idea of coming back it won't happen. He said you'll never take it up again, either do it now or forget it. Not that he wasn't going to help me get in, but he thought – and I think he was right. It's hard to go back after you have had a job somewhere and established yourself, and my job probably would have been in the army. I might not have returned from it at all.

But anyhow I always respected Dr. Cork very much. He was a very kind person, very helpful. He knew that we didn't have any money and he got me every job at the school I could handle. I was even night watchman when the night watchman was away. I cleaned microscopes. I had one job one summer working all summer on a course that he taught for the army, I guess the armed forces, because they were army, navy personnel on parasitic diseases and exotic diseases they would meet with overseas. This was right in the middle of the Pacific War and that paid me better than I've ever been paid before.

LS: Government work.

GJ: Oh yes.

LS: Can you say a little bit more about Rosebloom and what he was like?

GJ: Rosebloom, he was a very good person. I always liked Rosie very much. He was different from some of these – or rather I guess I don't know if you'd call him brusque, but he didn't sit around and chew the fat. He was all business and was a very, I think, well known person and well versed in his field and he did a lot of work – of course he went into the navy during the war and did a lot of work in the Caribbean area. I don't where else he was. He might have been in the Pacific some, but he was an admirable person and very good to me. He was kind of – in some ways kind of funny. He and I worked on – we had an insectary of course there and we had to pick the pupae everyday and Dr. Rosebloom would sit in the insectary with his arm in and there was thousands of mosquitoes on it feeding and he would slap the mosquitoes around says, "Geoffrey why the hell don't you pick the pupae so these mosquitoes aren't biting me all the time!" Feeding thousands on here, the one or two that bit him.

LS: I have a couple of more distant names. I just wondered if maybe you've come across them while you were there. E.K. Marshall who was in the medical school doing pharmacology.

GJ: Yup that's right. He was one of the early ones who got into the drug testing in malaria. Yeah I didn't know him well. I'd met him a few times and I knew that they were starting up that program and that program was a possibility for anyone coming out of the school for employment at that time if they wanted it. I did differently.

LS: You mentioned a couple of times the war coming into your graduate school time. Did you notice a real impact of the transition from peacetime to wartime on what was going on at the school, things that you could see, things that you had to do?

GJ: Yeah I think the impact mainly was I guess in the staff alterations that came about during that time. A lot of the staff went into the army; most of a lot of them had fairly high grades and high ranks in the army and the navy. So there was a turnover in some of the staff, nothing that really crippled the school. I mean Dr. T. B. Turner who was the bacteriologist or virologist, he went into the army. I guess, I don't know if he got to be a general or at least he got to be colonel, and was very prominent in the European theater, and others did go into the services. And the student population changed a lot. I think before the war they did a lot of training of Latin American physicians in Masters of Public Health and they'd come for a year. Rockefeller supported that program very heavily and I think that dwindled to some extent because -- well getting back and forth was not easy in those days, but it didn't really -- well, we all became air wardens and went up on the top of the school of hygiene and watched for airplanes and it wasn't that much of a change I don't recall.

We never knew where anybody was going when they left here because they were all going into the services in some way and good friends of mine, Allen Donaldson [spelled phonetically] was one who was there with me. He was there for two years and then left and went back directly into the sanitary corps of the army and right directly from there over to West Pacific area and he -- I guess he was gone for some three years. He came back and hadn't even seen child for -- until he got back three years later, two or three years later and he was a very good friend. And he and I crossed paths again here at CDC; he became deputy director of CDC for a while under Dave Censor [?] and he came down after he got back from his --

[break in audio]

GJ: -- I was just talking about Allen Donaldson who was a good friend of ours for many, many years. When he came back from his service and was let out of the army he didn't have anything to do, no plans at all. He came down -- we were living in Marietta, Georgia at the time and I was working at here CDC here in -- or MCWA and then CDC in Atlanta, and I encouraged him to go over to headquarters and see if they had anything that he could do and he did and they gave him an appointment. He stayed right in the Commission Corps and they transferred all of his army credits over to the Commission Corps and had no problem getting into that and he went up very rapidly in the CDC organization and was in the high administrative organization for many years and then was transferred to Washington where he became, I guess you'd call it, Rear Admiral. Got his admiral stripes and worked up there for a number of years and then retired and went out to Chicago and worked in the University of Chicago I think for a few years in public health there, and then died a fairly early death I think unfortunately. Well not terribly early, but early for what I look at it.

LS: Yeah anytime. When we spoke on the phone you'd mentioned that you'd been Hopkins over the years and that things had changed --

GJ: No not too many times, but I have been back a few times.

LS: But that things had -- looked a little different now.

GJ: I'm a dinosaur. When dinosaurs look what's going on today --

LS: I'm a historian so I'm interested in dinosaurs.

GJ: No I went back a few times, not too many and I've been invited to come back and Lloyd Rosebloom stayed at the school for many years and I've kept in touch with him pretty much and -- but I think the last few times I went back it's gone -- it's taken the same course that most research organizations have taken now, that the animal rooms and the insectaries are now replaced with computers.

LS: Yeah.

GJ: And so you walk down the halls of the fourth floor at the school of hygiene and you look in every room and somebody is sitting there looking at a computer and they aren't feeding mosquitoes or inoculating mice or anything of that sort. That was an impression. That probably is wrong but it -- I try to think I would have done the same thing if they'd been available at the time, but we were confined to the more real life of infecting animals.

LS: You had ducks and pheasants and...?

GJ: Oh yeah ducks, pheasants and I did a little work with mice, but not much. I learned all these things the hard way.

LS: From the whole animal?

GJ: Yeah. Well I don't have any trouble getting blood out of veins from people. I learned how to get blood out of the tail of a mouse. I think I could at least – I could get into a vein of a person.

LS: I was looking at – I just happened to have some of your dissertation and I was noticing that you'd gotten your pheasants from Allen Studholm [spelled phonetically]. Do you remember – I mean I'm interested in how you got materials for research then?

GJ: Well that's kind of vague but I do remember that we got a group of pheasants. I was trying a number of different – I tried a whole bunch of different hosts for plasmodium *lophurae*. I tried guinea fowl which you could buy at the market locally, and I heard that there were pheasants available at this Pennsylvania wildlife department, or whatever it was, and Studholm was just the name of the fellow that I – I never met him or worked with him. I may have corresponded with him and he shipped a couple of crates of pheasant.

LS: So he must have raising them for –

GJ: I think the – yeah, I think the state had a pheasant farm and they probably had more than they needed and they were willing to give up a few, and they were the wildest birds I ever laid hands on. They would actually beat themselves to death inside their cage if you didn't catch them in time .

LS: Wow, different from the computers I guess?

GJ: Oh yes, quite a lot .

LS: Okay, so after Hopkins you went to work for the Public Health Service?

GJ: Well, when I was at Hopkins, when I was finishing up and of course exploring what I was going to be doing and my draft status was going to turn into 1A from where it stood at the time, which meant that it wouldn't be long before I would be training at Fort Dix or someplace and Archie Hess [spelled phonetically] – I don't know if you've ever heard of Archie Hess. He was a biologist and was in charge of the malaria studies and biological studies at the Tennessee Valley Authority in Wolsamdam [spelled phonetically], Alabama. He was a good friend of Lloyd Rosebloom. Lloyd Rosebloom had worked at the TVA down there for a number of summers and he and Archie gotten to know each other very well.

Archie was looking for some – they were just getting into the study of DDT and the application of DDT as a residual spray inside of houses and as a aircraft spray as a larvacide. They had been doing it with Parascreeen [?], and he came through then and was looking for somebody to come work at TVA and I was interested in getting into the Public Health Service, which would've given me a commissioner officer status and saved my life as far as going to the Battle of the Bulge someplace, and so they sort of made the arrangement that I would go and work for the summer as a biological aid for Archie pending the acceptance of my commission – application for the commission corps. And so that's what I did. I got up every morning at four o'clock and went out into the field and waited for the aircraft to come over and wave flags and counting particles and measuring them and all sorts of –

LS: They were pelleting [?] DDT by then?

GJ: DDT yes. Well they got into some other insecticides as well, but DDT was the major thing and they worked in cooperation with the Department of Agriculture from their research station in Florida, Gainesville I guess – where was it? I can't remember. It was Gainesville or somewhere, and it was one of the early developmental programs on DDT and measuring the effects of it on – we sprayed experimental houses, as they did in all of the world finally, and released kegs of *Anopheles quadrimaculatus* into the houses and watched the knock down time and that sort of thing.

LS: So you're saying the plane flies over and sprays an area and then you look in the water to see what the particulate spread is?

GJ: Well we actually – we measured the larvacidal activity as well, but they were interested in -- they were developing a new method of spraying. Not just out of spray nozzles, but out of -- it was exhaust thing. I can't think of the name of the thing. My memory is getting bad. Well anyhow, the material is injected into a constricted place on the exhaust, which then expands and there's a name for it, but I can't remember. As it expands it shoots this out at high volume, high speed, and breaks it up into fine particles and the finer the particle the better it is as an adulticide, but not necessarily as a larvacide, because as a larvacide you want it to hit the top of the water and spread out.

LS: And they actually eat it, right, is that how it works or...?

GJ: Well it's contact. I think it's a contact spray and parastine [spelled phonetically] they had to eat, but this was a contact. They might eat it too, but it was strict and in wooded areas if you had a fine enough spray you could get the spray through the trees and get to the adult mosquitoes as well in their resting areas.

But anyhow that's basically one of the – a couple of the things we did. I worked with a number of people down there who were pretty well up in the field. Bob Metcalfe was one of them. His father was a professor of entomology at Illinois I guess. Bob Metcalfe was a graduate of Cornell. He was a very fine entomologist, medical entomologist I guess.

LS: You've mentioned to me that over time you felt a division of labor between the professional staff and the technical staff had changed or...?

GJ: I don't know. Do you mean at – in academic situations?

LS: Yeah or – but that – I think it was you were commenting on that the professionals – at a certain point what people did became divided out, and in the earlier period, people worked on whatever was at hand was the impression I got. Was that something from your TVA days that you were referring to or...?

GJ: Well, no I don't think so. At TVA, and I guess I've had sort of the idea when you're working for somebody you do what they tell you to do, and if they give you a task there aren't any excuses for not doing it unless you don't know enough to do it, but that has to become apparent if you're aren't going to do it. So I never had any problem as a professional. I – Bill Collins and I over the years have been the most highly paid pupae pickers that ever existed in mosquito insectaries. Ordinarily it's the sort of thing you turn over to a technician, but you don't find technicians that can do these things. It takes time to learn. So he and I have over the years done more of the scut work of an insectary and of mosquito rearing, and feeding, and inoculating than most technical people. That's the sort of thing that I've always figured that that's part of your job. Although I'm sure somebody would say it wasn't really cost effective.

LS: Probably not. So after Alabama you came up here to –

GJ: Yeah I was transferred to Atlanta in, I guess it was in 1945, in October I guess we came over here, and I worked in the – I guess they call it the Bureau of Laboratories and Training or something of that sort doing a number of things. We produced training materials, film strips, manuals, on various and sundry tropical and parasitic diseases, and they taught courses that brought in technologists from all over the country, the state health laboratories who wanted to send them, have them trained in the technology of the time. And parasitic diseases were sort of a new thing after the war. People brought a lot of them and began to be recognized as a real problem. So they had a fairly large program in training in the – I for a while ran what they called the extension service where we collected maybe a couple of hundred malaria slides and sent them out to various health laboratories, usually state health laboratories, all over the country. Then they would take a test or something of the sort to see if they were doing it correctly and then we'd sort of rate them on it and help them improve their skills if they needed it.

LS: So this is to help various local health –

GJ: Yes.

LS: -- diagnose returning soldiers and –

GJ: Yes. I think that the initial objectives of the CDC has changed somewhat in the recent years. Initially it was I think considered more of a service organization. They were there to serve the state health departments and they would send epidemiologists or laboratory people to the state health departments to instruct their personnel or they'd bring in for training, send the materials to use, and it was a major function of the CDC and they, of course, established their own bacteriology laboratories for diagnostic work and the virology laboratories were scattered through the country and did the same thing. So it was a big service organization at that time.

LS: You worked – go ahead.

GJ: Of course I was at CDC when it was formed on July 1st, 1946. So I'm a charter member.

LS: Did anything change with that or just the name at first?

GJ: It was rather strange. I think this had been seen coming for at least six months. I mean malaria control in war areas when the war ends, it kind of becomes obsolete doesn't it? So I think that some of the powers in Washington who wanted to see this continue as a unit got together and began to look at possible functions and somehow the budget system carried a whole lot of money through. I mean the war was over but we still had appropriations, so we began spending this money I feel sure because we began to receive all kinds of equipment and supplies which certainly weren't needed for the operations that been going on, and I think they supplied the new CDC with appropriations that were made for MCWA, that's just my guess. I wasn't in on that. I may be even entirely wrong, but it seemed that way because we began accumulating a whole lot of microscopes and film strip projectors and all sorts of things that were to be used in training.

LS: That sounds plausible to me, that description. So you were here in Atlanta and then still working for CDC went down to Puerto Rico –

GJ: Puerto Rico, right.

LS: -- at their tropical medicine school?

GJ: Yes. The CDC had – well I guess the MCWA had initially had established a small laboratory at the School of Tropical Medicine in San Juan to collect parasitic disease materials in large quantities to feed back to the group here in Atlanta who were sending these out to all the state health departments. Paul Weinstein, who was a good friend of mine over the years, was down there running that laboratory and he applied to Johns Hopkins School of Hygiene and Public Health for a doctoral program and they accepted him, so he was going back to Baltimore in September of 1946. So they were looking for somebody to take his place and since I wasn't probably going back to school anymore they sent me down and I was there for just about a year, and it was a nice experience and we – as I say we collected blood films, malaria blood films en masse [?] we collected *lophurae* [?] larvae on blood and stool specimens with various and sundry parasites in them, preserved them and sent them back. And so we provided an enormous amount of materials for the functions of the Atlanta group.

And I left there in guess about the 1st of September in 1947 and it was more or less for personal reasons. My father had become quite ill and my mother was anxious for us to get back on the continental United States. So I applied for a teaching job at the University of Bridgeport which was a brand new university which was spawned by the GI Bill mostly, and I was offered a position there of assistant professor. So I came back and moved the family in with my mother and dad. And we were there for about a year and I decided at that point that teaching really was not I had in mind.

LS: You were teaching biology?

GJ: Zoology biology, yes. In fact it was so brand new I was establishing courses for the first time in comparative anatomy and physiology and I didn't get to parasitology. They promised I'd get to that later, but anyhow after a year I went out to – not after a year, after a half a year, I went out to Chicago I went out to Chicago to the meetings of the American Society of Parasitologists, in fact I talked the school into paying part of my expenses, and while I was out there I met up with Martin Young and Bob Coatney who were looking for somebody to take over the Milledgeville Laboratory because Don Eyles was going back to Hopkins to get his degree. So I'd substitute for anybody who was going back to school. You know they just call me. And I thought that was a good opportunity. So I took it. I didn't even know where Milledgeville, Georgia was at the time. But I came down in June I guess, and the rest of the family followed me down in July, and we establish ourselves in Milledgeville. Worked with Don Eyles for about three months.

LS: Is that where you first met him?

GJ: No I had met him earlier back when I was in Atlanta when I first started out with the group in Atlanta. He was stationed at Swannanoa -- the Moore General Hospital at Swannanoa, North Carolina as part of the American Federal in War Areas Program on imported malaria. Actually he did -- he worked for the Public Health Services. It was joint -- the imported malaria studies was a joint study between MCWA and the Public Health Service.

LS: Did you work on that project when you were...?

GJ: No not directly.

LS: Not directly, okay.

GJ: But anyhow they were treating a lot of malarias that had -- they were working both on schistosomiasis and malaria and filariasis as far as that goes. So I went up there to see about collecting materials and actually we produced a training film while I was there and that sort of thing, and Don Eyles was there at that time and I spent about two weeks with him. That's the only time I had met him previously.

LS: But then you did overlap with him --

GJ: Yeah overlapped about three months and worked with him at a distance at least for many years after that until he died.

LS: Can you tell me about the set-up at Milledgeville? That was in the Georgia State Hospital?

GJ: It was in the Georgia State Hospital -- I guess they call it -- for the Insane. It was in an enormous instillation at that time. It was before the drugs had come along which kept a lot of people out of state hospitals, so there was somewhere between 10 and 12,000 patients at that hospital. In fact they outnumbered the town. I always felt that if a patient got to loose the town is in trouble. They had wanted -- they had asked the Public Health Services for some number of years to establish a laboratory there to administer the treatment of neural syphilis with malaria, which was what they were doing at Columbia, South -- the Columbia laboratory, had done since 1931 I think. And so they decided to take them up on the offer and they provided us with some fairly good space. Don had been there for I think about a year and a half before I got there, and it was a good place to work. It was sort of isolated. We were the only people in the area that knew what the hell we were doing, but it was -- it gave me a great opportunity to do some work with real malaria.

We had a fairly good-sized staff. There must have been -- I guess they ranged from six to eight technical people. There was only one professional. After Don left I was the only one left and one secretary, and we did a pretty large program of research.

LS: And you had good relationships with the medical people at the --

GJ: Mostly, yeah. Mostly. Some didn't like the idea of treating a disease with a disease. That was their major -- they felt that -- although they knew that a person with neural syphilis was not going to survive. Neural syphilis in those days was just about the same as AIDS now. You could just predict that if you don't do something they're not going to live, and they're going to have a long and terrible illness. So malaria was at that time the only treatment or the treatment of choice until the antibiotics arrived, and they began to use the penicillin and other antibiotics which displaced malaria entirely eventually.

LS: You said when we spoke on the phone that before penicillin treating a disease like syphilis gave you a rare opportunity to observe the disease.

GJ: Yeah I think so.

LS: Could you say a little more about that?

GJ: Well if one is looking at the natural history of a disease it's nice to know exactly what you inoculate, when you inoculate it, and follow the patient daily with the blood films, which are the indications of a malaria infection. Follow their clinical history hourly during the time they have an acute infection. And so it's just an opportunity to look at the natural history of the disease in its entirety, and you find out when the patient is infected with the mosquitoes and how long it takes for the infection to develop in those mosquitoes, and which mosquitoes are best and which are possible and which are not going to be infected.

So it was just an opportunity – and then if you build on top of that the testing of various and sundry drugs given in various and sundry regiments and dosages and taking a drug like primaquine, which is supposed to be a curative. How low of a dose of primaquine can you use in order to cure the patient. Now you can do this in prison studies but they're all short term. You don't have any long periods of observation. We observed some patients up to three years and followed their – without terminating the infection and we could see when the parasites came back whether it was asymptomatic or symptomatic and have a lot of very good records on long term observation of patients who had been inoculated with malaria.

LS: What was the relationship with like the work here at the federal penitentiary in Atlanta?

GJ: Well one of the reason the Public Health Service wanted to establish this group in Milledgeville was because at that time the program of testing drugs in the prisoners at Atlanta was opening up. There was another project done by Al Falding [spelled phonetically] up in Chicago, which was about to start. They had to have a source of infected mosquitoes and we provided that source for a number of years. Columbia had done some of it earlier, but it mostly fell on the Milledgeville to feed mosquitoes by the thousands on patients who were in infected stages of either – mostly *vivax*, a few *falciparum* cases and then – bring these infectious mosquitoes up to maturity and jump on an airplane and carry them to Seagoville, Texas or Atlanta, Georgia – we drove to Atlanta – or fly to Chicago, and we had our famous bite days were everybody would gather around and you fed the mosquitoes on the volunteers and then the mosquitoes had to be dissected to make sure they were positive and you finally came up with a positivity count, how many pluses you infected the patient with, and wait to see if the patient comes down or if the drug he happens to be taking is prophylactic or – what and I don't know how many patients we inoculated all together, a lot – volunteers I'm talking about.

LS: Right. So you had regular contact with the Chicago group, Alvin's group?

GJ: Oh yeah. Yes we went there every – oh maybe two or three or four months they'd have another batch of volunteers they wanted to test. They did – a lot of their work when I was involved in it was on the aminoquinolines, primaquine, penaquine [spelled phonetically], isopenequin [spelled phonetically], some of the newer ones they were trying out and I think they settled on primaquine as probably the best of the bunch and they worked on that for quite a while and I think it's not a well understood drug yet.

LS: No I don't think so. Did you have contact with the Rockefeller group at Tallahassee?

GJ: No. Only correspondence with Mark Boyd and the group down there. They did a whole lot of the same things that we did in Milledgeville and we always felt that theirs were not quite as detailed or as expanded as some of – where they would use one patient we would use ten and come up with the same results, but we thought you needed to use more than one patient. But I'm not critical of Mark Boyd's work because it was absolutely fantastic. He was a pioneer in that field and did remarkable things and the people that worked with him, too. I can't recall their names right now but ...

LS: You mentioned you did mostly *vivax* some *falciparum*, was Milledgeville a segregated hospital?

GJ: Yes it was at the time I was there, yeah.

LS: So you had...?

GJ: Yeah. Actually we did just about much work with *falciparum* at Milledgeville as with *vivax* because of the fact that I think probably 75% of the neural syphilis patients were black and would not accept the *vivax*. We'd used some *p. malariae* and we'd developed a strain of *p. ovale* which would in fact both white and black patients – the strain of *ovale* what we called the Donaldson strain, my old friend Alan Donaldson brought it back from -- probably from the Philippines although he might have gotten it somewhere else, but it was probably from Philippines and he kept relapsing and we finally picked up the parasite –

LS: Oh he literally brought it back?

GJ: Oh yes he brought it back inside of him. Right –

LS: Not frozen?

GJ: He called us one day and said, "I'm having a relapse. If you get people over here in a hurry I won't treat it." So we sent mosquitoes and people to draw blood and we picked it up. We didn't get it in the mosquitoes but we got a blood infection started in a patient.

LS: Something about Donald – mentioning Donaldson that means the tape is going to run out.

[break in audio]

LS: This is tape 2 of the Geoffrey interview. We were talking about Milledgeville, Al Falding in Chicago, Donaldson. Can we talk a little bit about Dr. Coatney? Was he involved directly with the work at Milledgeville and Columbia from the outset? What was he like; I mean what his involvement like?

GJ: Dr. Coatney came from, I guess Nebraska, and he was brought into the Public Health Service in 19 – I think 1938. He had worked on bird malarias and he had discovered a pigeon malaria that everybody thought was going to be a real tool in doing work with malaria and drugs and that sort of thing. So they recruited him and sent him to Columbia, South Carolina where Martin Young already was involved there, and I guess Martin Young had been there for a couple of years before Coatney arrived and they worked together there for not too long, a couple of years I guess. And then Coatney was transferred to Bethesda to take over some of the drug studies in mouse malaria and in the malaria work that they were doing in Bethesda. The war had begun and they were agonizing over the possibility of malaria being a problem with their source of quinine gone and no drugs to take its place except maybe adamarin [spelled phonetically] which turned everybody yellow and they didn't like that much.

So they were starting out a good-sized program. I think that it had already been going on and I can't remember the name of the gentleman who had been running until then, but Coatney was more or less put in charge of the development of that program, and he worked that into quite a large operation in Bethesda, had a number of people working for him, and doing work in the mouse malarias mainly and some of the bird malarias as well. Now he eventually got into the -- I guess into the organization of -- well it was a federal group composed of people in the army, navy, public health service, others that got together as a group to look at malaria studies I can't remember the name of them. Bill Collins has all the material on that. You probably saw most of it out there, and he, as the representative of NIH in that group, I guess he promoted the participation of the NIH in the studies on malaria and built it up to a much higher degree than it had started out. And in the process of that he got into the business of testing drugs and volunteers.

It started out, I think, in doing it at the Saint Elizabeth Hospital out at near Washington. They were treating patients for neural syphilis with malaria there as well and he was testing some of the drugs in those patients, but then they decided that was not really a very good source of information so they went into the prison volunteer system, and they had a lot of cooperation from the U.S. Bureau of Prisons and established the unit at Atlanta and they -- this unit in Atlanta, oh they had an entire hospital ward devoted entirely to the malaria studies and it was very well done and they assigned permanent personnel. Usually at least two or three physicians were assigned to the malaria studies at the prison, and they worked very well with the prison management and did quite a lot. And Coatney, he was the off site manager of that whole thing and spent a lot of time running back and forth to Atlanta because he dealt with the prison warden and this sort of thing and wanted to, I think, really manage the whole project for a long time.

LS: And you provided them with mosquitoes, also?

GJ: Yeah.

LS: And so on the drug development stuff work was also done at Milledgeville and then later done again at the prison or...?

GJ: Well no, actually we sort of paralleled. There wasn't any sequence involved because when newer drugs came along and we were beginning to use them in the patients, we didn't do any actual development of drugs. Pyrimethamine, for example, was developed as a new drug and Coatney always thought it was the most powerful drug we'd ever develop, but as soon as that had been tested enough in the prisons we began using it in the patients in various different dosages and regimens than they had used in the prison to see what value it was. We used chloroguanide another one we used, and of course chloroquine and amodiaquine, the 4-aminoquinolines we used a lot and combinations of the 4-aminoquinoline and the 8-aminoquinoline drugs were tried, and then the 8-aminoquinoline drugs by themselves were tested in some of our patients, mostly primaquine rather than the others, but to see what effect they had on the long terms relapse pattern of *plasmodium vivax*. The Chesson strain was one of the relapsing strains that we had used for many years and it was one that was most likely to relapse on a regular schedule, so we did a lot of work on those. But, like I say, it wasn't anything that they did it first and then when we did it or we did it first then they did it. It was a sort of parallel course that we took. As soon as drugs were available and judged safe then we used them in the patient population in the same way they used them at the prison.

LS: Did you know Sol McLendon?

GJ: Yeah. Yeah. Sol McLendon --

LS: He's the one that *falciparum* strain is named for?

GJ: Well one of the *falciparum* strains is the McLendon strain, yeah. No not – yeah *falciparum* that's right. Sol was on the staff of the state hospital of Columbia and there again it was a segregated hospital and there was an area they called state park which was, oh, about 5 or 6 miles outside of the city. This was the black part of the – Sol McLendon was in charge of that whole area for the black population. So he was a very cooperative physician and worked very well with – I never worked with him. He worked with Martin Young mostly and -- but he was still there when I was there and I knew him quite well, but he was a very good physician, took care of his patients very well and Martin always liked to include the physicians at the hospital in the authorship of the articles which he should have done because they were key to getting the data from the patients.

LS: Can you say a little bit about, speaking of strains of malaria, a little bit about the maintenance of different strains and the maintenance of the mosquito colonies and so on?

GJ: Well yeah, the strain situation arose during the imported malaria studies. I think they tried in that study to have strains from North Africa, South Pacific, from the Western Pacific, from Latin America – anywhere malaria was endemic they wanted to get strains, particularly of *vivax*, because that would be the relapsing parasite which might relapse back in U.S. when people came back from their adventures in those various areas. So a lot of strains were domesticated I guess you'd call it, and they had a number of outposts and a hospital out in California where we had a unit and one up in Newer [?] General Hospital in North Carolina and one in Texas I think and in a lot of places where people were coming back from the war to these various hospitals and developing relapsing malaria.

So a lot of the strains were picked up. Then we also picked up strains in the field. The McLendon strain was in a patient I guess at the state hospital that had been brought in for malaria in South Carolina. And so we picked up strains from them as well. I'd worked with the Santee-Cooper strain which was Santee-Cooper is a reservoir project over in South Carolina where they did a lot of work on malaria and that was picked up over there. The Chesson strain was a South Pacific strain and there were three or four other strains, the Tate [?] and a few others that were picked up in the South Pacific and which turned out to be standard strains that we used for many years. We picked up a strain of *falciparum* from Panama. We got somebody to send us blood from patients in Panama, which we then called the Panama strain, and we also domesticated *Anopheles albimanus* from Panama to see if it required *albinus* from the same geographic area in order to transmit the parasite, and it was a better transmitter than the domestic strains.

LS: So when you domesticated – were these kept and passaged in patients, were they frozen a mixture?

GJ: We tried to do as much mosquito inoculation as we could rather than blood inoculation, although if we could not find a donor for mosquito infections we would transmit by blood from patient to patient, and that was done a large part of the time during the early part of our studies until we perfected the methods for mosquito rearing and infecting and maintenance of the – protect [?] the mosquitoes and then re-inoculation of the patients, but both were used probably all in all maybe about equally, but I'm not sure if it comes out that way.

LS: Did you have any contact with the group up in Bethesda who were working on *gallinaceum* or other – the avian malaria people during or after the war?

GJ: Well not really. We had no collaborative studies with them, although I knew them and we always talked about the work they were doing as well as ours when we met at meetings and this sort of thing. Joe Greenberg was one of them that was up there and a number of others that worked for Bob Coatney and he had quite a few pharmacologist working for him because they were interested initially in their pharmacology, at least, anti-malarial drugs that were coming along. So I knew most of them and I knew their work was going on and they had a good insectary up there. Helen Trembly [spelled phonetically] I think was their – the manager of their insectary operations and reared a lot of mosquitoes for their use.

LS: You worked with bird malaria, with human malaria, can you offer me an open on the merits of animal malaria models, you know avian, rodent, primate versus human subject research?

GJ: I guess it's – it would be a sort of a progressive thing. We worked with what we had. We started out with bird malaria, because that's all we had and when plasmodium *berghei* and other mouse and rodent malarias were discovered, *berghei*, *vincke* and a few of the others, a lot of people went to the mouse malarias. I worked with them some, mainly in studies where we were investigating the possible collaborative infections with viruses and malaria parasites. That is, does the possession of a malaria infection enhance the possibility of virus infection and vice versa or does it enhance the pathology of infection or whatever? That was where we worked with *berghei* as far as I was concerned, but I didn't work much with bird malarias after I left Hopkins and got into the human malarias and then since then the primate malarias have come along. Well I haven't done very much in the line of personal involvement in the primate malarias. I've had a little, but when I was in charge of the laboratories that were almost entirely working with primate malarias.

LS: Milledgeville was your operation and it was shut down in '54 or so?

GJ: Yeah.

LS: Why was it terminated?

GJ: Well it was a decision made by the powers in Washington, actually. They were condensing their budgets, their laboratory and everything, and there was a trend in those years to get rid of field stations. Field stations were a bad word. They -- they were a cash cow; I think they felt that cash was flowing out from the headquarters and NIH to a little place down in Milledgeville and nobody was going to really get any benefit from that. You couldn't go to Congress and say, "Look, we're in Milledgeville, Georgia." They went, "What the hell are you doing there?"

Anyway, I think it was more or less just the fact of condensation and amalgamation of -- and for one thing the number of patients to be treated with malaria were diminishing rapidly. The antibiotics had come along and the only patients we were treating were those who had failed with the antibiotics at that point. There were quite a few of those, but anyhow it was a limited source, and the same thing was true in Columbia where I was sent. After Milledgeville closed there was a trend toward broadening the field into work with intestinal parasites and other parasitologic fields which were very appropriate for a mental institution of course. And so malaria was -- malaria was eradicated. You don't remember when malaria was eradicated?

LS: I've read about it. I've heard that it was eradicated.

GJ:

LS: Well in this country.

GJ: In this country essentially it was, yeah, and this was something that damaged the budgeting of funds for malaria because you had to go to Congress and say, "Well we've eradicated this disease but we need a heck of a lot of money to work on it."

LS: So you worked in these populations with intestinal protozoa?

GJ: Yeah and [unintelligible].

LS: Had you taken -- you know -- did that harken you back to Hopkins days? You take a protozoa zoology course there?

GJ: Oh yeah. In the department of parasitology at Hopkins you were immersed much more in the intestinal parasite field than you were in the nice clean malaria mosquito thing, because all the course -- and of course I was -- I assisted for two years in the teaching of parasitology courses so I got to be fairly well acquainted with intestinal parasites going down to the abattoir and collecting a bushel of ascariks [?] from the hogs. And so I felt right at home. I didn't particularly enjoy it, but I felt right at home.

We did quite a few field studies and a lot of studies in the population and also looked at new drugs for the treatment of parasitic nematodes mostly, and they -- at one time you know they decided they were going to have re-educate me. They sent me back to Yale for a year for an M.P.H. to work in the field of arthropod born viruses. So when I went back from that I started working with the possible combination of arthropod viruses with malaria parasites. I worked with Saint Louis Encephalitis and a few of the others, also LCM viruses.

LS: Was that -- oh, would you say it wasn't entirely voluntary going for the M.P.H.?

GJ: Well no, it was voluntary. I guess maybe I was looking for some kind of new horizons. A couple of years before I had asked to go over to London, to the London School for a year to spend with P.P.C. Garnum and that group, and it was all set to go and then finally the budget figures came out and Dr. Willard Wright, at that time he was in charge of the parasitic diseases laboratory at NIH. He was a very nice person. He called and said he was very sorry but they had X'd out my year in London. So I had the privilege later on of sending one of my good friends over Dewey McWarren. So he did a year with Garnum. So vicariously I felt that I had been satisfied.

LS: So your work at Yale then related to virus and...?

GJ: It was almost entirely in the virus field. Well I took the M.P.H. course and got an M.P.H., but I was sort of assigned to the blood borne viral laboratories there, and I worked with them and learned a lot about to handle viruses, how not to handle viruses.

LS: Can you tell me a little bit – sort of moving slightly backward in time, but a little bit about the transition from Milledgeville to Columbia, South Carolina and what was different about your job, about the place?

GJ: Well it was somewhat different. You know you get used to doing your own thing and you finally end up with somebody telling you what you can and can't do, but Martin Young was not a difficult person to work for. He was very friendly and very – he was encouraging to anybody to do their own thing if they wanted to, but Martin at that time the malaria potential was diminishing to the point where I think Martin told me, and I guess he told Bill when he came in that do whatever you want but malaria is mine. So but I spent the first couple of years I guess, probably half of my time, of writing up results from the Milledgeville laboratory and getting that into publication and reporting that material. So I didn't really get out of the malaria field that much.

LS: So you brought the records with you from Milledgeville?

GJ: Oh yeah. They still exist.

LS: I think I saw them yesterday.

GJ: You did. I had them before I moved out of my house up here I had them all in file cabinets in my house and then I knew I wasn't going to have any room here for them and so I – Bill and I had worked with them for a long time already and so I took them all out and Bill has them now.

LS: I'm going to come back to that – to that project of yours with Bill. You had mentioned that you were often the replacement guy. You mentioned it today earlier usually when people went to school, but you replaced Martin Young?

GJ: Well yeah, eventually Martin Young left for Bethesda.

LS: To the extramural program?

GJ: No he was going to be Dr. Coatney's Assistant Director of the Laboratory of Parasite Chemotherapy [?] and that's the job he took. I don't know if I – Dr. Young and Dr. Coatney were very good friends when one of them was in Columbia and the other one was in Bethesda.

LS: That worked well for them?

GJ: That worked well. You put the two of them together – and I think Dr. Young lasted for about two years and then went over to extramural programs and that was where I replaced Dr. Young again.

LS: Oh so that's when you moved –

GJ: That's when I moved to Bethesda. So Dr. Coatney wanted to travel. The unit had been established out in Malaysia and he wanted to travel and his chief, I think it was Dorlan Davis [spelled phonetically], told him, "Okay you can go, but you're going to have get somebody to run your laboratory while you're gone," and Martin had already departed. So I got notice that I'll give you two weeks and then you're going to be transferred to Bethesda. Now I went. In the Commission Corps that's what you say, "I go where they tell me to go." It was kind of inconvenient because my family couldn't finish school -- this was in March or April and of course I couldn't take my kids out of school and my wife had to stay and sell the house all by herself. So it wasn't easy, but I went and I was Dr. Coatney's assistant and he went off on his travels and I don't know what I did to handle the place but I was there.

LS: So then you were working directly with...?

GJ: Directly under Dr. Coatney.

LS: And in the same city.

GJ: Yeah.

LS: Was that different for you? Did you have the same concerns that –

GJ: No.

LS: -- Dr. Young had?

GJ: I was – actually I was assistant director to Dr. Coatney in the LPC. All of the intramural programs in Bethesda under that thing were mine. He was more or less out of that. He did the stuff over in Malaysia and down in Atlanta and everywhere else and he talked to the director of the institute and I didn't have to. So the whole the program was under me and I worked well with the people who were there and I think we accomplished quite a few things.

LS: So – go ahead, I'm sorry.

GJ: That's all.

LS: No I was – so that was pretty much an administrative position?

GJ: Yeah it was.

LS: -- Gradually doing more and more administration?

GJ: Oh yeah that came with time. The administration is inevitable unless you purposely avoid it, which Bill Collins has been able to do.

LS: That's why I raise the issue with you, because he's very proud of his ability –

GJ: Oh yeah he should –

LS: -- dodge it.

GJ: -- he has dodged it and he's made a great name for himself and reputation is world wide in excellence, but he did not want to be an administrator and I don't blame him. I didn't either but I didn't have the opportunity – I didn't have the perhaps the knowledge that Bill Collins has. So I think he can do it well. I might not have.

LS: You still had contact with a lot of the science and the scientists.

GJ: Oh yeah! Well I didn't divorce myself entirely from working in the lab because I did work with a number of the people and we published on various subjects. My name was not on a paper at that time just because I was director of the laboratory. I only published things that I worked on. Since then Bill Collins has stuck me on a whole lot of things.

LS: So you were there until – you were eventually Chief of LPC.

GJ: Yeah, Bob Coatney retired. They made me acting chief for a year or so and then decided to make me chief and then there was a – Captain John Segal [spelled phonetically] was the one that – he was director of the – what was his title?

LS: I don't know.

LS: I think it was Scientific Director for the institute I think, but I'm not sure. Anyhow he had an office in our building and he was a good friend too, and they decided at some point to sort of reorganize the whole thing. Earlier on, when Dr. Willard Wright was there – he was a fine person and he had the laboratory of parasitic diseases I think, and when he retired there were I think three people in that laboratory who were never going to get together in the same laboratory. So they split it up. Parasitic diseases went to Leon Jacobs. They established a laboratory of parasite chemotherapy for Bob Coatney and then the germ-free animal studies went to Lloyd Newton.

And so the whole group got split into three and they got along pretty well that way. So that's how the LPC came into being and I think there had been, after Coatney quit or retired, I think the – and Leon Jacobs had also retired, Paul Weinstein [spelled phonetically] was in that position at that time – they decided, I think, to put it all back together again, get rid of this fragmented thing and that's what they did and that's when I – they didn't put it all back together. They made a laboratory of malariology I guess now Miller is chief of or something like that.

LS: Ian Miller and [unintelligible] I think.

GJ: Yeah, I'm not sure how they're organized now, but anyhow that was when I decided to retire.

LS: That's the first time you retired? That was your retirement from NIH.

GJ: Well I retired from the Commissioned Corps. I had 25 years and I had four kids in college. Do you know what my salary was when I retired [telephone ringing] – I'm sorry. Hello?

[break in audio]

LS: I think we're good to go. You were talking about retiring.

GJ: Okay I was saying I was retiring from the Commission Corps and I was making a gross of \$20,000 as a four striper in the Commission Corps at that time. That was in 1969. The pay has gone up considerably since then I hope.

LS: I know probably I would imagine so.

GJ: But anyhow with 4 kids in college and not wanting to incur a lot of debts I decided to retire, and at that time I could take half of my retirement, and I had an offer from CDC to come back to CDC as a civilian employee, civil service on an AID supported -- A-I-D supported project that was the laboratory down in Central America.

LS: So that's USAID?

GJ: Yeah. And this was the Central American Malaria Research Station they called it at that time. So I accepted that with great glee.

LS: Back to the field station.

GJ: Back to the field station. They my pay starting there was some \$10,000 more than what I had left off and I was getting half retirement. My kids thought they'd died and gone to heaven . I could pay all their college expenses and they all finished college without one cent of debt.

LS: That's always a good number.

GJ: They have thanked me for that because they have kids now who are going to college and at much greater expense than mine and they find out that it's not hard to incur a lot of debts in college. So that's how I left CDC – or NIH.

LS: So you were four or five years down in...?

GJ: Four years, from 1969 to 1974. Well four and a half years about.

LS: What kind of work did you do there?

GJ: Well it was a laboratory set-up in an area that was having great difficulties with the so-called eradication programs. They were not working. They had – this was a country and an area, which concentrated a great deal on the culture of cotton where they did spraying by the ton of all kinds of insecticides and every mosquito in the country you had to hit it with a hammer to kill it. You couldn't kill it with any of the normal insecticides that were available. Plus the fact they had tried mass distribution of drugs, combinations of chloroquine and primaquine, and they had real problems with the acceptance of these mass distribution programs so they hadn't achieved anywhere near real coverage.

So USAID and PAHO [?] and others decided that they needed to have more research, field research, so they established a station down there which had been there for a couple of years when I went down. Don Plesh [spelled phonetically] had managed it and he was leaving, and it was – we had a fairly large staff and a couple of entomologists, a couple of parasitologists, a medical officer, myself and several others off and on over the years, and a fairly large technical staff, plus I guess maybe a staff of 25 or 30 field workers with a lot of vehicles and attempted a fairly big program of testing new insecticides, new methods, surveillance methods of finding out how much malaria there was and how much we needed to do to get rid of it and I'm not sure that we ever accomplished anything, but we learned a lot.

LS: So it was both surveys and also testing?

GJ: Yeah. I think we – one of the major things we learned was something that nobody really wanted to accept -- that eradication of malaria was not going to be possible in that area with what we knew and had available at the present. That's all.

LS: It's hard to teach people what they don't want to learn.

GJ: They don't want to know that. We would not dare mention that at a PAHO meeting .

LS: This is already the early '70s when there's –

GJ: Yeah.

LS: -- you're not the only place that might be giving them those sorts of indications?

GJ: That's right they were hearing this a lot of places, but they always managed to point out the successes, which had been success. For example, I guess in Sri Lanka and India they were down to just a few thousand cases. Well now they're back up to a few million and, but, they were successful for a while but it was an enormous effort. You just can't maintain an effort like that over the years, so it gradually creeps back up. Anyhow we learned a lot and I think we gave them their money's worth.

LS: So that field station was shut down –

GJ: No.

LS: -- or you just came back to Atlanta?

GJ: After a couple of years they changed the name of it. After two or three years I guess, USAID lost some interest in funding the place and Dave Sensor, who was the director of CDC at the time, said, "Well if they're not going to fund it we'll fund it ourselves." We changed the name to Central America Research Station, CARS, and they began to send down epidemiologist to look at other problems, intestinal parasite transmission problems and whatever turned up in the area they had people down there working on, and they established a medical group and we built ourselves a little clinic down in the thing where they could see what sorts of diseases and medical problems were important to the populations in the [unintelligible].

LS: So you interfaced then with the local public health?

GJ: Yes. We always had very good cooperation from the local public health people and, of course when he went there, there was a very definite malaria eradication program, which was sort of separate from the regular public health system. We worked with them for many years but we kept our contacts with the regular public health organizations in the country.

It was a well-supported outfit. We were one of the major participants in the embassy protocol and we were a member of the country team and that sort of thing, so we were taken in and given all the support we'd ever really need from the embassy. So it was a nice experience; we enjoyed it. My wife thought she'd go down there and die, but after the first six months she began to enjoy it very much. Well when you have two maids living in, a gardener –

LS: It's a different style of life.

GJ: It's a different life entirely. Yeah we had a beautiful house down there. It was a nice experience.

LS: So then you came back to Atlanta?

GJ: Yeah. After and in February 1st of 1974 I guess I was transferred back to Atlanta to work in the – under Dr. Kaiser. I was assistant director of the – he ran that –

LS: Was it division of tropical diseases?

GJ: Tropical diseases I guess, yeah. It had been the malaria division. It was the division of tropical diseases and I worked at Clifton Road for about a year I guess, a year or so, and bought a house right up the street here so I'd be right near Clifton Road and they transferred me up to Chamblee, but that was fine because in traveling when you drive from here to Chamblee in the morning all the traffic is coming in the other direction.

LS: Right you're outbound, yeah.

GJ: And at night the same way.

LS: So was there a culture difference or anything between –

GJ: No, no. I had – actually when I was with NIH in charge of the group up there we had our own group down here at Chamblee. Bill Collins was here at the time. He moved over from Columbia when that closed, and that closed very shortly left Columbia, and they moved them all over here and they began working with the primate malarias and they, the CDC, they bickered with CDC for building space. My old friend Alan Donaldson was here at the time and he was very helpful and he and I have been good friends for years and he got us some very good quarters and the rest of them were very cooperative, too. Dave Sensor was director at that time and so we had a fairly large group here raising mosquitoes and working with the primate malarias and the prison project was still going on and they were trying to infect prisoners with the simian malarias, successfully mostly, and then Bill did work on viruses here as well and that developed into a long number of years with Bill doing productive things .

LS: And you retired the second time in 1984?

GJ: '84, yeah that's when I decided to hang it up. Probably too early, but –

LS: You want – in retrospect you wanted to stay longer?

GJ: Not necessarily, things had not developed along the lines that I'd like to see and I was glad to leave.

LS: Administratively? Scientifically?

GJ: Well, organizationally.

LS: Organizationally?

GJ: Yeah.

LS: Okay.

GJ: I think – well as a working scientist, or I considered myself a working scientist, we became infiltrated with a bevy of epidemiologists whose aim in life was to travel. Whenever there was something going on in Africa, we sent a couple of our epidemiologists over. And so it became epidemiologically oriented which I was not particularly fond of, but Bill Collins stuck it out and he did very well. But I felt ready to retire anyhow. I felt that I wasn't contributing that much to their new departures. So I – and I kept on consulting with WHO and AID for a number of years after that. I was on the field mal group as a member of the field mal group in Geneva. I went to Geneva twice a year.

LS: Field mal is?

GJ: Field investigation – it's part of the TDR group. Malaria had three different units. It had chemotherapy, immunology, and field investigations; chem mal, imm mal and field mal. Well field mal was the one that took care of all the field investigations and we granted monies to people, all those who applied for assistance, met twice a year and I was on the steering committee with that and for about five or six years after I left, I think somewhere around there, and that gave me some nice travels. They sent me to meetings in Africa and to a field study in Brazil and over to Australia and New Guinea, Bangkok, and I finally before I retired I guess I was – went to China for about a month under the US China agreements in malaria. I was the first US person to go over to China as a part of that agreement. It was a very interesting trip.

LS: So you've mentioned your work with World Health Organization, you did various things with them from the '60s thru the '90s. Do you have any lasting impressions of the organization and its evolution over that period?

GJ: My participation was mainly as a member of the various scientific groups that were convened for particular purposes, scientific group on the treatment of malaria, scientific group of the immunology of malaria and that sort of thing and they were fairly specifically oriented, and I had really very little to do with any administrative part of the WHO. I was always in awe of their administrative structures and how they worked. They seemed to be overly supplied with people, under supplied with money, but the US provided them with most of their support I guess in those years and maybe still do, but it was interesting because I got to – other members of these scientific groups were people that I had known for years or wanted to know for years. I met an awful lot of very nice people, very good scientists.

LS: And so those panels then – the international conferences you think those really help as far as exchange and...?

GJ: I think so. I always thought so. I don't know how they're looked at now, but we had the international congresses of malaria and tropical diseases, or tropical medicine and malaria I guess you'd call it. They were separate for a long time and I attended a few of those and they were monstrous things. You did get to hear a lot from foreign scientists who were interested in the same thing and got a chance to talk with a lot of them. I always felt they were worthwhile but maybe I was young and liked to travel see exotic places and that sort of thing.

LS: You also did – you mentioned USAID; you did consulting for them on Africa, Haiti and –

GJ: Yeah, yeah I took a couple of trips to Haiti to evaluate the programs there. They were supporting, at that time, many programs in many American countries as well, not so many African, but a lot of American countries and Asian countries and they did evaluations occasionally every four or five years I guess and they convened a group that they thought might be able to look at what was going on, what the results were and what they should be doing in the future and then make a long report which probably [unintelligible] nobody ever paid any attention to them, but that's just my impression.

LS: How – go ahead.

GJ: Go ahead.

LS: No, how did that compare with like WHO or...?

GJ: Well –

LS: And they're both big bureaucratic --

GJ: -- they were somewhat different. WHO was – had a more diverse group at the top. They had a director and he knew all of his people who worked under him and some of them were people that I had known for years so I felt on an equal basis with my status, but AID – USAID, was more of a dictatorship. We had people like Ed Smith who ran the malaria programs for many, many years out of AID and Ed Smith was – he was revered by many people in the foreign countries where the programs were because he was the one that said yes or no or here's your money or forget it. So they became a bit – I probably shouldn't be saying this, but I don't want to cast [unintelligible] but they were – they thought they were doing a good job and I think they did a good job basically. You felt sort of like you were working in a dictatorship than a democracy.

LS: As an administrator you didn't have sympathy for that style or...?

GJ: I don't think I was ever a very good administrator. I always asked people what they thought and I probably got most of my good ideas from other people – I didn't – you don't generate ideas all by yourself. You've got to either read or listen or talk to people and if any of your ideas come out to be good you can't take that much credit for them.

LS: So in that way you're more sympathetic with the WHO organization –

GJ: I think so –

LS: -- diverse –

GJ: WHO had a lot of people who had good jobs with the WHO, but not have had such good jobs that they had to go back home, but that's true of a lot of organizations.

LS: Probably yeah. I wanted to move back to the work that – you and Dr. Collins have published quite a few papers in the last – in recent years largely by mining old data from –

GJ: Well yeah that's right –

LS: -- from the previous –

GJ: Most of the recent –

LS: -- five years [?]

GJ: -- I guess probably in the last ten years this has been going back into the archives and looking at the results that we got way back and answering some questions which -- I don't think we went back and looked for questions. I think we had questions and went back and looked for answers and we could find it in this data that still exists. I went through a whole bunch of different phases when we were in Columbia we had to get out of malaria a little bit and into some of the fringe area, and he did a lot of work on the transmission of viral diseases in mosquitoes and then all of a sudden along came the first antibiotic method for looking at malaria infections, Miller and -- the immune status of patients. Probably the FA method -- IFA and that became very important at Columbia because we had here again a mine of patients who had been there for years who we knew had had a particular malaria five years ago or ten years ago or twenty years ago. We could get blood from them and subject it to a study by various and sundry immunologic methods, and IFA was the one we were mostly interest in. So Bill did a lot –

LS: Immunofluorescent antibody assay?

GJ: What?

LS: IFA.

GJ: IFA, yeah and then he went into the -- later we got into a different one. I can't think of the name of it.

LS: I probably have it.

GJ: Yeah, but anyhow, Bill did a lot of work with sera he collected from patients at the state hospital in Columbia.

LS: So some of this was actually going back to the patients on whom you had the old data and actually collecting fresh –

GJ: Right –

LS: -- sera to study.

GJ: Yes and actually even after Bill moved over to Chamblee I think he and one of his technicians went back to Columbia, South Carolina and searched out patients whose name they had and took blood samples from them. It's very likely that that is the sort of study that would not be approved now by any ethical committee although we -- at that time we didn't consider it to be unethical to go back and study a patients blood sera even though we hadn't planned to do it to start with.

LS: Yeah I mean you're looking at like cross immunity between -- I mean if you were to design a study now you'd have to infect someone with multiple species of malaria over time and that would be –

GJ: Yeah, actually I think if we -- the way I see it now if we had a battery of sera that was collected ten years ago for one purpose and we decided now, hey this will be real valuable if we can go back and look at it for another purpose we would not be allowed too. That's my impression. I haven't kept up with the ethical conditions that exist these days. They've changed a lot.

LS: Dr. Collins mentioned a couple of sets of ethical review that the project went through. Other people are interested in this data I gather also. Do you have any contact with them?

GJ: Oh yeah Bill has – he's been hounded by people for this data, McKenzie [spelled phonetically] I think is his name up at Harvard has been doing a lot of analysis of the data and we have a group over in –

LS: Tubingen.

GJ: Tubingen, Germany Klaus – I can't think of his –

LS: I can't remember his name right now either.

GJ: Anyhow Klaus something. Anyhow I went over there to a meeting and Bill wasn't able to go and I went over in his place and probably poorly represented him, but we talked about the data and they have done quite a lot of analysis. He's in the high tech computer type stuff which is beyond me, way beyond me, and has come up with some very interesting conclusions, but – and I think Bill said that he's heard from him recently that he wants some more so he may be going further with it.

LS: The other – I think it's interesting to me that you and Dr. Collins, both at these various places where you did this work, saved all the records even if that meant bringing them home for a while and you don't see – I mean people like James at Horton, I mean there must have been other places where people would have had this kind of data but the question is did any of it –

GJ: Where is it?

LS: -- survive, yeah. I mean it's really –

GJ: I don't know why decided to do this except that I guess we just had them. When I left Bethesda I packed up all the data I had and put it in storage here in Atlanta thinking I might eventually be back with CDC somewhere where I'd want to use it. In fact I packed up all the – every slide we ever made at Milledgeville were in slide boxes and I packed all those up and kept it for years and years and years. And in fact when I went out to Chamblee they'd fill up a whole room in one of the trailers out there, and Bill and I look back at that now and we kick ourselves because he and I went out there one time and we dumped those things in the trash, and the technology has caught up with us. He could have used those slides to determine many things that – well I guess characteristic of the patient and the patient's history, but they're gone .

LS: Yeah. I think the stuff you saved is -- you did better than most.

GJ: Well it's documentation – I worked hard on that documentation. Technicians who spent hours making very good records and I hated to see it trashed.

LS: You mentioned earlier that this kind of research would not be approved today or even the reuse of stuff – can you say a little about how patient consent has changed over time.

GJ: Well I don't know. I haven't been involved in the recent past in this, but when we were doing our studies at Milledgeville we had patient consent or consent of the person who was responsible for the patient. That was a given. We had to have that before they were inoculated with malaria; the patient had to give consent or his sponsor from the outside or the courts or whatever it took. They were awarded. They had to be – it was a general consent. The same thing that you sign when you go in for surgery. "I give this physician permission to take out my gallbladder." I didn't – you didn't have – you don't even now have to be very specific, that I want him to use so many stitches and this all you wanted to do was to give him the general consent to do what he has to do and that's what we had.

We had consent to administer treatments which were necessary for the well-being of the patient and we didn't have to explain what the well being was or how it would effect the patient otherwise, but we were expected to be careful with what we did and to not do anything that was out of line, whether we were at times I don't know, but we were pretty careful and we always went back to the fact that you were giving this patient malaria, but he was in pretty bad shape already and we saw a lot of cases where they recovered and that almost made it worthwhile.

LS: And the chemotherapy work had an [unintelligible]?

GJ: Well yeah I think that – I guess on that we went more on the basis of not to do any harm. We knew what the limits were of a particular drug and we were not going to go exceed these limits. And so we gave drugs at different regiments at different amounts per day – different – whether it was three times a day or once a day or – that was most of our investigations not to give the drug its usability but to determine how best to use it.

LS: A couple of things that we didn't touch on that I just wanted to move. They're not exactly in chronologic order. Can you say a little bit more about Don Eyles and primate – and his primate work? I mean I know you weren't directly involved but just your impressions of him.

GJ: Yeah, Don and I were good friends for years after I'd worked with him for a few months and after finishing at Hopkins he was assigned over to Memphis, Tennessee. Where –

LS: That was another field station?

GJ: Yeah that was another field station that we'd had for many years. In fact that was one of the original field stations from the Public Health Service back in the 1920s and '30s. The malaria investigation branch or something like was under Louis Williams and Washington had established that and the prince was in charge of it for a while and when I was in Alabama I went over there to visit to actually get a physical examination and an interview because I was going into the Commission Corps and Vic Haas was in charge over there at that time of course Vic later became director of the National Microbiological Institute at the NIH. And so – and they worked with bird malarias, *gallinaceum* mostly, and they did some field studies on malaria in the area and also contributed their expertise to the medical school there in microbiology and that sort of thing. What was I –

LS: Don Eyles.

GJ: Oh, Don Eyles, yeah. Well Don Eyles was transferred to Memphis after he got through with Hopkins. He was supposed to come back either to Columbia or to Milledgeville. They felt at that time that malaria work was diminishing so Don was transferred over to Memphis to work on toxoplasmosis and whatever malaria was left to be done in the bird malarias and that sort of thing over there and Don did a whole lot of work on the treatment of toxoplasmosis, the epidemiology of toxoplasmosis. Don was a great field person. He loved to get out and one of his projects was to go out and pick up dead buzzards and find out –

[break in audio]

LS: No, no I promised you I would warn if you were rambling too much. Okay this is tape three of the interview with Dr. Jeffery. I think we're working. You were talking about Don Eyles.

GJ: I was talking about Don Eyles working on toxoplasmosis at the Memphis laboratory and one of the things that I mentioned was that he was a great field worker and didn't stop at anything much. He collected dead buzzards to look for signs of toxoplasmosis and he did quite a lot I think very worthwhile work in that field, but kept on with some of the malaria studies that were out there in *Plasmodium gallinaceum* and moved a little bit into the primate malarias. He was looking at some of the *cynomolgi* strains in rhesus monkeys and during the course of this more or less minor study he and I guess another one of his technologist or technicians came down with malaria and it proved to be *cynomolgi* malaria which was perhaps the first report of this species of monkey malaria being transmitted into man through mosquitoes and that raised the specter of the possibility of malaria being a zoonosis which would obviously have a great deal of effect on the evaluation of eradication programs if people were going out and getting monkey malarias and that would not bode well for the eradication of malaria. So he pursued this study further and they moved the study also over to the human populations of the prison and found out that they could with some ease transmit *Plasmodium cynomolgi* to prisoners.

Don Eyles then was sent over to Malaysia as I recall, a TDY, thing to see what possibilities there were of setting up a study in Malaysia since that was an area where a lot of different simian and primate malarias existed?

LS: What's TDY?

GJ: Temporary duty, sorry . He was just going over for exploration of the area and came back from there after a few months. I'm not sure how long he stayed out there, but he came back and recommended that the institute of medical research or the IMS – anyhow an institute that existed in Kuala Lumpur was interested in having a study done in their area in cooperation with them, the primate malarias of the area and their possible infectivity to man and vice versa I guess, malaria in primates. So a [unintelligible] long [?] study was established. Dr. Coatney worked his miracles and got a lot of money appropriated for the study and they established the laboratory over there, which, Don went back over and managed. I can't remember the number of years, but quite a few years and a number of other people worked with him over there. Chuck Dopravali was one. He took a number of –

LS: Who?

GJ: Chuck – Charles Dopravali. He worked with Bob Coatney also on malaria projects in Guatemala and elsewhere and had been at the Chamblee laboratory here for a while and Mack Warren went over to join them later on. They worked with a lot of people who were also working at the institute there on malaria problems. Some of them were Australians. They worked with – I can't remember all of their names, but he did a really very excellent study on the primate malarias in the area in the apes and in the monkeys of the area. And it developed into a program that demonstrated the likelihood or the lack of likelihood of primate malarias being important in the viral eradication programs and I think probably they came to the conclusion that while it was possible to transmit malarias to man, but probably it was not going to be a likely event variable for very many places but anyhow it was a good study.

LS: You mentioned the buzzards and I saw yesterday – and this is a digression also – two pictures of Don Eyles that Bill Collins has in one he's evidently out with his gun bearers in Malaysia hunting monkeys I guess that was one of the ways of collecting.

GJ: Yes.

LS: And then in another he's out in the countryside somewhere dressed like an explorer talking to some farmer. I mean he seems like a fascinating guy.

GJ: He was a field person. Besides being a good malariologist and parasitologist he was a superb botanist. He collected – he specialized in sages and he collected those. He had a marvelous collection of those things and I remember one time when we were together in Milledgeville, Georgia we were driving over to Columbia one day. We went over there quite often to talk to Martin Young about various problems and all of a sudden he pulls up along side the road and says, "Way back there's a marsh that has a sage that I don't think I have" and he struck off across the fields carrying his little botany collection kit and came back – actually I walked with him and we came back in a little while and he was fully satisfied you know collected a sage.

LS: I think Bill Collins mentioned yesterday that Don Eyles on his honeymoon discovered like two new plants or something.

GJ: He and his wife honeymooned up at a lake over in – near Memphis, one of those cut offs from the Mississippi. I can't remember the name of the lake, but it's a very well known one and they described a couple of new species and published it after their honeymoon was over.

LS: Interesting guy.

GJ: Yeah he was. He was a very bright guy, but he unfortunately did not live out a full life. He died of a heart attack but I'm not – I wasn't terribly surprised because when I first met him, when we were at Columbia together he chain-smoked cigarettes and chain drank Coca-Colas. The girls over at the lab never could put down a Coca-Cola they were afraid that Don would come up and drink it . He was – he wasn't too careful with his health I think, but maybe not he was [?].

LS: I also heard a story he was the first one to get – the first documented person to get a primate malaria through mosquitoes I heard a story about someone else getting a naturally – it was a naturally occurring human infection with *knowlesi*, did you ever hear anything – out of Malaysia or something? Does that ring a bell?

GJ: Yeah. That happened some years after the project out there I think, but I can't – I don't know much about that. It's vaguely in my mind that did happen, but I was more sure that it was more recent than –

LS: I think it is, yeah. I don't – I'll look into it more.

GJ: I think Bill Collins told me about that recently.

LS: I have two questions just moving back into what we talked about earlier. You say in El Salvador there was some resistance to drugs that were being brought in by the people that were supposed to use them?

GJ: Well talking about resistance I didn't mean to imply there was any drug resistance –

LS: [unintelligible]

GJ: -- the populations are always hard to control.

LS: Like compliance issues?

GJ: Compliance mostly and it was – the distribution of drugs in a given area, they tried quite a few programs of that where they hoped to reach a 100% coverage. In fact it almost required 100% coverage in order to be sure that this was going to be a method for actually eradicating the infection in the area and getting 100% compliance is never easy. In fact I think maybe it's almost impossible even when they did small pox eradication it was difficult at times to get 100% compliance and then they finally got enough I guess to push them over the edge but that was one of the big problems in the Central America or in any area where they were trying to use mass drug distribution as a method of controlling eradication.

LS: Was it specifically to the new drugs? Were people used to quinine or just general...?

GJ: I think it's just general orneriness. You tell me to take a drug. I ain't going to do it. They aren't – often they're not given enough information, that was one thing -- we had a young woman working as a health educator in the center down there who went to a great deal of trouble to find out why people did not accept drugs, and I think she found out that often they were not given enough information. They would just sort of say, "Take this or else." They were -- don't ask why, just take it and she felt that it would have been -- well it might not have been possible but it would have been better if they had been able to better educate the people and let them know why they're taking it, what it means, but it's very difficult in a population as poor and as hopeless as some of the populations were in that particular rural area, those particular rural areas, but it cured a lot of malaria, kept a lot of people from being ill, but it didn't ensure eradicating and if you stopped the drugs it's right back where it started.

LS: I want to move us back for a minute, back to Syracuse, and I just had a question about the canaries. Do you know where the canaries were – Manwell got his canaries and...?

GJ: I have a feeling he went down to the local 5&10 and bought them as a pet.

LS: In the pet trade?

GJ: Yeah.

LS: That's just one of the things that I'm personally interested in how –

GJ: I know that has been the source of a lot laboratory -- in fact when we first started working on the primate malarias here in Chamblee Bill would go down and buy squirrel monkeys for like \$10 each at a pet store, and how much do they cost now? Probably \$2,000.

LS: He was saying, yeah, they're really – once people have caught on that there is a market for them.

GJ: But he bought them at local pet outlets. Some of his South American monkeys, they were available and they worked fine. I – they used squirrel monkeys at NIH for a lot of – not just for malaria but for a lot of experimental purposes. I remember going into a squirrel monkey colony that they had at NIH. There must have been hundreds of them. Taking blood film from every one of them to see if they had any natural malaria infections that they brought with them from wherever they had imported them. I don't think we found any, but the monkey situation over the years has deteriorated to the point where it's almost impossible to get a monkey and when you can get it you can't afford it.

LS: You've got your guinea fowl in Baltimore, I guess –

GJ: -- the market.

LS: You buy them live? Yes, otherwise they're no good to you. That's harder to do now, too. I think that was it for my questions. I wanted to ask you if there's something I should have asked you about today but didn't. Something that occurred to you while we were talking or something –

GJ: I can't think of anything. I think I've rambled through almost everything I could think of.

LS: I'm sure I've missed a few important ones. Are there any people I didn't mention? I didn't mention a lot of people, other people that you've worked with. For instance I know the Hopkins crew because I've done some work on the history of the Hopkins program, but at other places I don't know the names and are there people from Milledgeville or NIH or CDC that I should have mentioned?

GJ: Yeah, I think probably in a conversation like we're having I'm inclined to omit the effect that other people have had on the work that I've done over the years. There are too many. You can't include them all. I've worked with maybe eight or ten physicians at Milledgeville who were very good friends, very helpful, they were the ones that – I was not a medical officer. I couldn't do anything on the medical side, so they -- I had to collaborate with them on every patient we had. If the patient was to be treated I talked to the physicians and say, "I want to treat him with chloroquine at such and such a dosage," and he wrote the order. And so that was – that was one group both at Milledgeville and Columbia we worked very closely with the physicians on the wards and they were extremely helpful and instrumental in being able to do the work. We couldn't have done it alone, and the director of the hospital itself was very sympathetic to our needs and helped us with whatever we needed.

In places like the NIH there were cross communications among everybody there who was working on a parasitic disease. We met frequently in seminars and that sort of thing and I knew a lot of people in NIH that I didn't work directly with but were influential in my career I suppose. I knew a lot of the medical officers that worked at the Clinical Center in the infectious disease area. Did studies with them quite often. The administration at NIH is – you don't include all these people; they all have a big effect on one's career. Dr. Justin Andrews was Director of NIAID for a long time. I had known Justin Andrews for a long time. He was a Hopkins graduate and was director of CDC here for a while. I was not at CDC when he was director, but I saw him a number of times when I was passing through. Those are the people that I guess that you leave out.

LS: Yeah, what was Justin Andrews like?

GJ: Well he was a very – I think he was probably as much of an intellectual as I've ever met in the field. He was very keen on any problems that came up in parasitology he could think through them better than almost anybody I know and a good administrator I think. He made good friends and handled his staff very well and I admire – am a great admirer of Justin Andrews, and of course he was a Hopkins graduate so that got a good start.

LS: That's all I can think of right now. There's nothing – no one else we're missing?

GJ: I don't think of anything.

LS: I mean just move – one – to something you just said, you were commenting about how good [?] all these people use interacted with when you were at NIH, and earlier you had said that Milledgeville was a little isolated, so I guess there isn't – the field station versus the central thing. I mean there are intellectual and scientific and social advantages?

GJ: Yeah. We always – when you're at a field station like Milledgeville or Columbia you sort of feel like you're shut out from the major source of money, influence, the administrative attention that sort of thing. It sounds like my wife has returned. Is that you?

Mrs. Jeffery: Hello? It's me.

GJ: Hello. This is Dr. Slater and my wife Jane.

[break in audio]

GJ: We won't bother you with our –

Mrs. Jeffery: Okay, good –

[break in audio]

GJ: No the – are we going?

LS: Yup, we're going.

GJ: The field station mentality is something that I think I've seen in many places and you feel abandoned sometimes and you feel that people don't understand what problems are in a field station when they're talking to you from Bethesda, Maryland which is, you know, the Mecca. But all in all I've always got good cooperation. Sometimes some lack of understanding of what the problems with the field stations are. You're sort of left out there out on your own to do things that you don't get any encouragement from anybody about. So there is a difference and I sometimes I resented it.

Bob Coatney would call me from Bethesda during the time that I was providing mosquitoes and I was not – I didn't sit on swivel chair and dream all day. I was out on the wards taking blood films and feeding mosquitoes and doing the whole thing along with all the rest of them, but he had called a couple of times and was out. So about mid-afternoon he called back again and got me and he says, "What are you doing? Out playing golf all morning?" Now that's the sort of thing that – he should have known better. I think he was trying to be funny, but it didn't sit very well with me.

LS: Doesn't translate well over such a long phone line.

GJ: No that's right. Well anyhow I got generally got along pretty well with Bob Coatney he was not easy to get along with, but I managed. Everybody else did –

LS: What was he like?

GJ: Well Bob Coatney was – I can't say a lot against Bob Coatney because he was very good to me and he provided me with support and things that I needed to do things that I wanted to do, and he was not easy to get along with in a lot of ways. He was pretty much self-centered I think, and he dealt in the higher echelons of the scientific world. Sometimes didn't have an awful lot to do with the people who worked under him. But then, on the other hand, if an award came up that he thought one of his people should be named for he nominated them. If there was an office in a society that he thought somebody – some of his people would do well in he nominated them for that. He was always looking out for the interest of the people that worked for him in a sort of a higher echelon way, but when they did something day to day it wasn't so much of a concern. But he was unique.

LS: Can you tell me a little bit about working with Bill Collins? I mean I know I just spoke to him yesterday but...

GJ: Well, Bill and I have worked together a long time and I enjoyed Bill right from the start because when he came to Columbia he was anxious to do things out in the field as well as in the laboratory. In fact he was out collecting catalpa worms and catalpa [unintelligible] so he could bleed them and get their fluids out so he could use it to rear insect tissue, and I'd go out with him and we'd pick up these monstrous big –

LS: These are caterpillars?

GJ: Yeah they're larvae of the caterpillars. Very big green things, you know, and we'd go on to find the catalpa trees and go under and pick them up and other things like that. He and I would go out and collect larvae out at the sewage disposal plant where the overflow was breeding larvae and we were trying to find out if any of these larvae would yield any kind of viruses.

LS: These are mosquito larvae?

GJ: Mosquito yeah. And we did a project – these are just examples that I think indicate my communications with Bill. We had a project down in Bluffton, South Carolina, around the coast, where there was a rural community which had intense infections with intestinal parasites, diaserus [spelled phonetically] and phocerus [spelled phonetically] and [unintelligible] and that sort of thing, and we were testing some new drugs for treatment of these things and we worked with a physician down there and we went down about once a week to collect stools, and this was in the mid-summer it would be 150 degrees in the shade at Bluffton, South Carolina and humidity was even higher and Bill would go along. He wanted to go along and see what we did and worked with us and he'd collect the stool specimens and we had to put them into iceboxes to keep them and then I had to run them back to Atlanta – or to Columbia in the afternoon trying to drive fast enough to escape the order which was in the trunk.

But he enjoyed that kind of thing. He wanted to go out into the field anytime he could; he wanted to know what everybody was doing and he was always helpful anytime he would work with you. He wasn't a parasite. He was an active worker and always ready with a new idea, and he jumped onto the immunofluorescence study, the IFA study, with great zeal, recognized the fact that he had materials that nobody else in the world would have and he kept on with that. And he was always ready to go into something new and participate in something new and he was always a giver. People all over the world have said they've gotten things from him and he's been willingly giving them to him. When we were in Central America we would send back hundreds and hundreds of blood specimens and he'd run the IFA on them for us and he participated. He was a part of the project but he was always willing to do something to facilitate any project you had. So basically he was a person that you couldn't dislike. His personality is such that you could always have fun working with him.

LS: He's a nice guy. He gave me a present too yesterday so...

GJ: Is that right?

LS: Yeah and I just met him.

GJ: Well he's overly generous with – Mac Warren and I have been friends with Bill for a long time. We were always after him, quite doing all those things for people. Do some things – they never recognized him. Do some things for yourself. They're going to bleed you dry if you keep on offering to do studies and offer to give slides and sera and the whole works. He's – but he's overly – I wouldn't say overly generous. He's very generous and he's that type of a person that you get a lot from.

LS: Can you tell me a little bit about Mac Warren?

GJ: Well Mac is another individual – it was funny that Mac Warren graduated from the University of North Carolina with a Master's Degree. He worked for a biological supply house for a year or so and hated it a lot and then found out there was a position available at Columbia under Dr. Martin Young. He said he worked as a GS-1½, it wasn't that low, it was a 3 I think, a technician, and worked in the mosquito colony and Martin Young wanted to see people advance.

Mac was a very well educated person. He had a Master's already in parasitology under John Larsh [spelled phonetically] who was another interesting schoolmate of mine at Johns Hopkins and Martin called him and asked him if he didn't want to go for graduate work and I guess Mac said he wanted to some time. And he said, "Well where do you want to go?" Mac told Martin, he said, "Well I'd kind of like to work with –" the guy at Rice, a famous molecular parasitologist.

LS: Oh I'm drawing a blank, okay.

GJ: It was at Rice University and Martin knew him – Chandler.

LS: Chandler?

GJ: Yeah and Martin knew him very well so he -- he picked up the phone and called Chandler and said, "I've got a candidate for a doctorate would you accept his application?" And Chandler said, "Sure I'll send him the papers," and filled them and he was accepted. He went out to Rice and got his PhD out there and from there he went over to Oklahoma to work in the medical school there and taught parasitic diseases and microbiology and other things and then when the thing developed in -- so I knew Mac when he was a GS 3 . But when the thing developed on Malaysia we were looking for somebody to go out there and so they called Mac to see if he would be interested and he said, "Yes!" And off he went to Malaysia to work with Don Eyles.

So I was -- I worked with Mac from a distance because I was sort of at that time running the front office for the Malaysia thing after Bob Coatney had left and Mac and I corresponded a lot. When he came back from there he worked for a while here in Chamblee and then I got him to come up to Bethesda since I needed an assistant director of the laboratory and I wanted somebody I could rely on. So I got Mac to come up there and he came up and worked with me there for a while and then he went on to -- he went to London for a year and worked with Professor Garnum and came back, and when I retired and went to El Salvador Mac told me he would like to come down and work down there.

So I got Bob Gieser [spelled phonetically] to snatch him away from NIH and send him down to El Salvador and we worked there for three or four years and when I came back he was director of the laboratory. They replaced him with a medical officer within six months or so and he came back here to work with me at Bethesda -- at Chamblee. So and then -- he went -- he got out of the field of malaria for a while. He went into administrative work at CDC; they wanted somebody to run all the scientific services division at --

[break in audio]

LS: You were talking about Mac Warren and administration work.

GJ: Mac Warren, yeah, yeah.

LS: CDC.

GJ: He went into administration there and so -- he and I have known each other for a long time we've kept up our friendship for many years since I retired and since he retired finally. He became editor of the American Journal of Tropical Medicine and Hygiene and sort of reorganized their editorial policies and now he's retired and lives up in New Hampshire with his wife and we visit him every once in a while up there. If you can get through the snow drifts.

LS: Yeah I need to get up there and talk to him, too.

GJ: Well he's a delightful person. He's very knowledgeable in the field and he's had a lot of experiences and I've always enjoyed working with him administratively and scientifically; he's been very good.

There's one thing that I didn't get to in the questions -- you asked about [unintelligible] we had uncovered. When Don Eyles left for Malaysia he had been appointed Secretary Treasurer of the American Society of Tropical Medicine and Hygiene. So he had to resign and they were looking for a new one and I volunteered. I guess, or somebody volunteered. I think Martin Young volunteered for me. So I was made Secretary Treasurer of the American Society of Tropical Medicine and Hygiene which I held for six years and sort of ran that society. That's before they got real fancy and got a commercial concern to run the --

LS: Right, still member run back then?

GJ: -- entirely. I didn't get a salary for it. I employed my wife as a secretary and they paid her -- they started out when she was -- they paid her \$100 a month to serve as my secretary; she worked hard for that. And then we were at an annual meeting and I had to report the -- I was at the council meeting and reported the expenses of the Secretary of Treasury Office and the secretary is \$100 a month and that's \$1200 a year. Tom Weller [spelled phonetically] was there. He looked at me and he says, "You employ your wife full-time for \$100 a month?" I said, "Yeah that's all the society will authorize." He said that is an absolute disaster. I move here and now that we raise that to \$300 a month and they did .

LS: That was nice.

GJ: So it was nice and Tom was actually a good friend, but she worked hard for that \$300 a month. God she sent out – did the address [unintelligible] for every publication that we sent out and the news and the whole thing and it was hard work, but that put my kids through college. And I was elected President of the society a couple years later, 3 or 4 years later. I can't remember 1975 [unintelligible] I appreciated that, but anyhow.

LS: Did you go regularly to the meetings even when you're not an officer?

GJ: Well I haven't gone in the past few years. We had go occasionally, but it has kind of passed me by.

LS: But before you went –

GJ: Oh every meeting every year, every one I could get to.

LS: That's all that came to you?

GJ: That's [unintelligible]

LS: I wanted to ask you about one more person who you mentioned, Joe Greenberg.

GJ: Yeah I don't know Joe very well except that he was one of Bob Coatney's protégés during the time that they working heavily on *berghei* and the mouse malaras they were doing some physiologic studies on that and doing some pharmacologic studies on the drugs that were used for treating the malaria and I met Joe a number of times, talked with him when I was in Bethesda and was visiting up there, and we talked a lot about malaria problems, but I never really knew him too well. He was a nice guy. He was very capable I think. I don't what – he lives somewhere around here, in this area. I've seen a time or two since I've been in the Chamblee area, but I don't know an awful lot about him. But I think he was very capable.

LS: The *gallinaceum* stuff I think is an interesting thing. It sort of overhung the war project and went off until they published a big set of results in like –

GJ: Yes.

LS: -- this was sort of the last hurrah of avian malaria as a major research tool.

GJ: A lot of the publications that were – they were publishable during the war but they were restricted. You weren't allowed to publish a lot of them so a lot of them came out as soon as things loosened up and so I – there were a lot of – there were a lot of publications out of the Columbia laboratories out of the laboratory of parasite chemotherapy. Bill Collins has collected a lot of these and –

LS: Yes.

GJ: And provided me with a number of them. I won't get up because – I'll show you some of the volumes when we finish; they were published right after the war and you've probably seen them already, but there were some on the imported malaras. That's about all I can offer.

LS: Okay. I think that's quite a contribution.

[end of transcript]

